

Correspondence

Do anaesthetists need to wear surgical masks in the operating theatre?

The article by Skinner and Sutton¹ has recently come to our attention and we have read it with great interest. The authors examine the role of masks in the operating theatre both for the protection of the patient as well as for the protection of members of the health care team. They reach the conclusion that surgical face masks do not serve either purpose.

While we applaud Skinner and Sutton's challenge of operating theatre "sacred cows", we have significant concerns regarding the review of the literature and purported "evidence-based recommendations". The literature critically examining the role of the mask in preventing surgical site infections is sparse. Aside from a single study from Sweden² and in spite of the title of the current paper suggesting there is an "evidence base", none of the available literature withstands the rigors of a critical review. Specifically, with the one possible exception noted, the few studies available are not randomized, and do not utilize standardized and recognized definitions prospectively applied to risk-adjusted patients monitored in hospital and following discharge. That is, none meet the standards of modern Infection Prevention and Control. An excellent critical review of the available literature was published recently³.

The study by Sheretz et al of the "cloud adult"⁴ serves as a sober reminder that while many of us harbour *Staphylococcus aureus* and while most of us do not shed most of the time, when incubating or suffering from viral respiratory infections, we may become prolific shedders surrounded by a swarm of potentially pathogenic microbes. As there are typically four to eight individuals involved in a surgical procedure and the average adult experiences two to four viral upper respiratory tract infections per year, there is significant potential for contamination of the theatre airspace, albeit on an intermittent basis. It may well be that masks in the operating theatre provide little benefit most of the time but that during sporadic periods of high risk shedding, significant benefit is to be had. The role of the cloud adult in the transmission of hospital infections may be underestimated⁵ but only carefully designed, long-term prospective studies can address this notion and the potential role of surgical masks in prevention.

Of most concern to us, however, are the statements

and quasi-evidence attributed to our hospital. On page 336, we are told that masks were eliminated for non-scrub staff in 1993 based on "published evidence" (though no reference is cited) and that "infection control monitoring in the Royal Alexandra Hospital shows no sign of increase in the rate of post-operative infection". As this information is provided to support elimination of masks in the operating theatre, it must be reviewed carefully.

Prior to 1993, the infection control program in the Royal Alexandra Hospital was lab-based and relied primarily on monitoring of culture results. There had not been any study of mask efficacy or prospective surveillance for surgical site infections, nor was there any "published evidence" to guide a decision to abandon masks. Rather, based on a review of the evidence similar to that presented by Skinner and Sutton and a conclusion that little data supported the use of masks, masks were no longer compulsory for use by non-scrub staff (at the discretion of the surgeon). This decision was driven primarily by various opinion leaders in the surgical suite who, given a lack of evidence supporting their use, simply decided that they no longer wanted to wear masks in the operating theatre. It must be noted that both circulating and scrub Operating Room personnel were required to wear masks during procedures requiring implantable devices such as orthopaedic joint prostheses and vascular bypass surgery.

As there were no prospective surgical site surveillance data prior to 1993 to compare with, there is no way to know that there is "no sign of increase in the rate of postoperative infection". While a study has been proposed to the Department of Anaesthesia to examine the issue of surgical site infections with and without use of surgical masks, no such study has ever been undertaken. The statement that infection rates are not "significantly different from comparable regional institutions performing similar procedures" reflects wishful thinking. No data exist to make this comparison. Moreover, we are extremely concerned about the source of such information. No source is cited. Infection Prevention and Control Services at the Royal Alexandra Hospital was not contacted by the authors to discuss mask use at this hospital or to provide this information.

The issue of masks in the operating theatre was revisited at the Royal Alexandra Hospital in 1997.

While the Infection Control Committee acknowledged the paucity of data available supporting or refuting use of the mask in the operating theatre, in the absence of additional data or in the absence of a clinical trial approved by the regional ethics committee and with informed patient consent, it was felt that the hospital must abide by Association of Operating Room Nurses (AORN)⁶ and Operating Room Nurses Association of Canada (ORNAC)⁷ recommended standards and practices which require use of masks by anyone entering the surgical suite when open sterile items and equipment are present.

As the data attributed to the Royal Alexandra Hospital do not exist and as we do not support the statements attributed to us, we respectfully request a retraction of the paragraph of this manuscript that describes us as a "precedent ... to discontinue use of surgical masks".

In these times of cost-containment and evidence-based medicine, we wholeheartedly support careful review of traditional and accepted practice. We further acknowledge, as noted above, that the evidence available is limited and that the issue of whether masks do or do not prevent surgical site infections is not settled. We agree with Romney³ that more well-designed studies are required before a change in current practice can be endorsed. An anecdotal observation from our mask experience is that as long-accepted practice changes, other unanticipated changes may follow. As standards relax, the operating theatre may become a more casual "affair" in which increased visiting and talking ensue. The opening and closing of doors and disrupted traffic patterns may be far more relevant to outcomes than the presence or absence of masks but only carefully conducted studies will control for these possible confounders and resolve the mask issue once and for all.

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Do anaesthetists need to wear surgical masks in the operating theatre?: reply

Thank you for the opportunity to reply to comments raised by Joffe and Lafferty.

We agree that definitive Level 1 evidence is lacking to determine whether surgical face masks serve a definitive purpose in either protection of the patient or the anaesthetist. What was reviewed in our article was the *available* evidence with recommendations made on that evidence.

The study by Sheretz et al does serve as a reminder that we harbour and shed potentially pathogenic organisms. We also agree that a well controlled long-term prospective randomized study would be useful in elucidating the role of the "cloud adult". On the current evidence, should any theatre/operating room occupant who has viral or other upper respiratory tract infection be barred from entry?

We were surprised to read that "while a study has been proposed to the Department of Anaesthesia to examine the issue of surgical site infections with and without use of surgical masks, no such study has been undertaken, "and" as the data attributed to the Royal Alexandra Hospital do not exist and as we do not support the statements attributed to us, we respectfully request retraction of the paragraph of this manuscript that describes us as a precedent ... to discontinue use of surgical masks".

We regret that Joffe and Lafferty are "extremely concerned about the source of such information ..." and that a retraction has been requested. The information quoted in our article was supplied during research and has come directly from the source. We quote in part from the correspondence received from Theatre Nursing Management, Royal Alexandra Hospital. "... after much discussion about the pros and cons of eliminating the use of masks, the medical officer in charge of Infection Control at our institution gave ... the go-ahead to change our practices regarding masks. ... The change released the circulating nurses and anaesthetists from the need to wear masks in the theatre during cases. ... This change of practice caused an immediate uproar in our suite which was fuelled primarily by the vascular and orthopaedic surgeons. ... It was decided that a modification was necessary and we proceeded to require

masks in theatre in which implant surgery was being performed. Our policy has remained the same to this date ... we have NOT demonstrated a higher rate of infection (since introduction of no masks) ... as compared to other comparable institutions in our region for similar procedures”.

In addition to the above we quote again from this correspondence “... the decision to eliminate masks was not taken lightly by our hospital, and we continue to generate discussion due to it ... OR nursing standards continue to reflect the necessity of mask use and we have been subject to a great deal of pressure thus far, because the evidence *against* our practice is no more convincing than ours is in favor of it!”.

We regret that the information supplied cannot subsequently be supported by the suppliers. Our aim in this review was to stimulate discussion and set a base guideline on the data that are/were current and like Joffe and Lafferty, we will be keen to resolve the mask issue by carefully conducted studies.

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More reports of spinal cord damage by spinal needles

We wish to draw our colleagues' urgent attention to a rising incidence of spinal cord damage from spinal needle insertion, usually as a part of a needle-through-needle CSE (combined spinal-epidural) technique, but also with single shot spinal blocks. Reynolds reported (from the U.K.) seven cases of damage to the conus medullaris following spinal anaesthesia in 2001¹ and another report in the same issue of the same journal described two cases arising in one institution². In the past 12 months we have reviewed medicolegal reports of a further three cases in Eastern Australia, with additional anecdotal reports also being received.

Most of the reported cases have been in obstetric patients, who experienced marked pain during spinal needle insertion and subsequently developed persistent unilateral sensory loss and pain in one leg, together with foot drop in the postpartum period, sometimes with temporary impairment of bladder function. MRI scans in the majority of the patients have shown a characteristic syrinx or cleft at the base of the spinal cord, in the conus medullaris at T12-L1.

Reynolds attributed her cluster of cases to the use of the L2-3 interspace for spinal needle insertion and suggested that even a normally placed conus, ending

at L1, could be damaged by this approach using atraumatic needles¹. Her recommendation that spinal needles should not be inserted above the spinous process of L3 should be stressed and the utmost care taken in the identification of the appropriate interspace. However, it has been repeatedly demonstrated that it can be difficult to reliably identify a particular lumbar interspace by palpation of the commonly-used landmarks, in particular the iliac crests¹.

Reynolds also recommended that if spinal needle insertion causes pain, injection should be avoided, although it may be too late to avoid nerve damage¹.

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Phonomyography—Acoustic myography using condenser microphones: a promising new method of monitoring neuromuscular transmission

We thank Dr Torda for his detailed review on fundamentals and new developments in neuromuscular monitoring. We would like to provide additional—and critical—insight into acoustic myography, a new and, as we believe, very promising method of monitoring neuromuscular transmission.

The first description of the fact that contraction of thumbs close to the ear stems from 1663 and was written by the Jesuit priest Francesco Maria Grimaldi in his book “*Physicomatheis de lumine*”. A muscular aetiology of these sounds was proposed by Wollaston in 1810¹ who, by comparison with sounds he was hearing whilst traveling London's cobblestone streets in his carriage was also able to define the peak frequency of these sounds in the range of 25 Hz—long before Fast Fourier Transformation was developed! Modern interest in this phenomenon started in 1980 by Oster and Jaffe². During the following decade, plenty of research provided detailed insight in the nature and physical principles of acoustic myography³⁻⁵. The acoustic oscillatory-type waveform is produced by lateral movement of the central regions of the muscle⁴ and occurs at a frequency corresponding to the resonant frequency of the muscle³. Non-muscular related aspects of monitoring these sounds affect amplitude and frequency of the sounds,

such as the distance between the microphone and the muscle or the pattern of stimulation and the origin of stimulation (voluntary—active or passive mechanically or electrically). Therefore the power spectrum derived from voluntary muscle contraction is different from the one derived from physiologic tremor where peaks occur between 6 and 8 Hz⁶ or electrical single-twitch stimulation⁷. So far, there are only three original articles which used air-coupled chamber microphones^{8,9} or small condenser microphones⁷ to measure neuromuscular blockade. We have used the term “phonomyography” (PMG) rather than acoustic myography to describe the determination of neuromuscular blockade using a condenser microphone. We have determined the peak frequency of a single-twitch derived signal at the corrugator supercilii muscle as in the range of 3-5 Hz using Fast Fourier transformation and found it an easy-to-use method, providing stable baselines and very little disturbances due to artifacts⁷. The most important finding, however, is the good agreement with mechanomyography, as found for the larynx, with a bias of and narrow limits of agreement. This is much better than the agreement found in earlier studies^{8,9} which were in the range of 40%, as we believe, unacceptable for research purposes and even clinical needs. Dr Torda rightly stated that the usefulness of acoustic myography needs to be determined. The current studies undertaken at our institution are very promising and indicate that phonomyography (=acoustic myography using direct contact condenser microphones)—derived pharmacodynamic results and mechanomyographically derived results show very good agreement¹⁰. We would like to draw the readers’ attention to follow these developments closely

because we believe phonomyography to be of special interest not only for research purposes but also for routine clinical use.

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